

RELATIVE REINFORCER RATES AND MAGNITUDES DO NOT CONTROL CONCURRENT CHOICE INDEPENDENTLY

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One assumption of the matching approach to choice is that different independent variables control choice independently of each other. We tested this assumption for reinforcer rate and magnitude in an extensive parametric experiment. Five pigeons responded for food reinforcement on switching-key concurrent variable-interval variable-interval schedules. Across conditions, the ratios of reinforcer rates and of reinforcer magnitudes on the two alternatives were both manipulated. Control by each independent variable, as measured by generalized-matching sensitivity, changed significantly with the ratio of the other independent variable. Analyses taking the model-comparison approach, which weighs improvement in goodness-of-fit against increasing number of free parameters, were inconclusive. These analyses compared a model assuming constant sensitivity to magnitude across all reinforcer-rate ratios with two alternative models. One of those alternatives allowed sensitivity to magnitude to vary freely across reinforcer-rate ratios, and was less efficient than the common-sensitivity model for all pigeons, according to the Schwarz-Bayes information criterion. The second alternative model constrained sensitivity to magnitude to be equal for pairs of reinforcer-rate ratios that deviated from unity by proportionately equal amounts but in opposite directions. This model was more efficient than the common-magnitude-sensitivity model for 2 of the pigeons, but not for the other 3. An analysis of variance, carried out independently of the generalized-matching analysis, also showed a significant interaction between the effects of reinforcer rate and reinforcer magnitude on choice. On balance, these results suggest that the assumption of independence inherent in the matching approach cannot be maintained. Relative reinforcer rates and magnitudes do not control choice independently.

Key words: choice, concurrent schedules, generalized matching, reinforcer rate, reinforcer magnitude, key peck, pigeons

Choice in concurrent variable-interval variable-interval (VI VI) schedules is controlled by the relative rates, magnitudes, delays, and qualities of reinforcers obtained from each alternative, and by the effort or force required to emit the operant response on each alternative (Baum & Rachlin, 1969; Davison & McCarthy, 1988; Killeen, 1972). Baum and Rachlin suggested that the effects of all these independent variables on choice might be represented by a single multidimensional strict matching equation (Herrnstein, 1961), later generalized by Killeen and now known as the concatenated generalized matching law:

$$\log \frac{B_1}{B_2} = \sum a_X \log \frac{X_1}{X_2} + \log c. \quad (1)$$

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In this equation, B measures responses emitted or time spent responding, X measures each independent variable (IV) in turn, and the subscripts 1 and 2 denote the two concurrent alternatives. a_X is the sensitivity (Lobb & Davison, 1975) with which choice changes as each IV is changed, and $\log c$ measures inherent bias, or any constant residual preference for one or other alternative that remains when the effects of all IVs have been included.

According to Equation 1, each IV exerts its effect on choice in the same way, via the log ratio of the values of the variable on the two alternatives, multiplied by a sensitivity parameter. If any IV is constant and equal on the two alternatives, it drops from the equation because its log ratio equals zero. Thus, for an experiment in which reinforcer rates on the alternatives are varied across conditions, but reinforcer magnitudes, delays, and so on are always equal on the two alternatives, Equation 1 reduces to the familiar generalized matching law (Baum, 1974):

$$\log \frac{B_1}{B_2} = a_R \log \frac{R_1}{R_2} + \log c, \quad (2)$$

where a_R is sensitivity to reinforcer rate. If any IV is constant, but unequal between the alternatives, it contributes a constant amount to bias, or the intercept of the fitted line, because its log ratio is constant and nonzero. For example, if reinforcer rates were varied across conditions, and the reinforcer magnitude on Alternative 1 was always twice that on Alternative 2, Equation 1 predicts that choice will be described by Equation 2 with an additional constant bias equal to $\log 2$ multiplied by the sensitivity to reinforcer magnitude.

Many experiments have examined Equation 2 as a description of behavior when reinforcer rates are varied, and the typical finding is slight undermatching (Lobb & Davison, 1975) or sensitivity values between 0.8 and 1.0 (see e.g., Baum, 1979, 1983; Davison & McCarthy, 1988; Davison & Nevin, 1999; Taylor & Davison, 1983 for reviews.) In contrast, the other IVs listed above have received less attention.

For an experiment in which reinforcer magnitude is varied and all other IVs held constant and equal, Equation 1 reduces to:

$$\log \frac{B_1}{B_2} = a_M \log \frac{M_1}{M_2} + \log c, \quad (3)$$

where M measures reinforcer magnitude, and a_M is sensitivity to magnitude. The general result of the small literature on varying reinforcer magnitude is strong undermatching, with typical values of a_M around 0.5 (e.g., Davison & Baum, 2003; Keller & Gollub, 1977; Landon, Davison, & Elliffe, 2003; Schneider, 1973; Todorov, 1973; Todorov, Hanna, & Bittencourt de Sá, 1984). That is, changes in reinforcer magnitudes have less effect on concurrent choice than do changes in reinforcer rates.

Schneider (1973) varied both the reinforcer rates and reinforcer magnitudes (defined as number of food pellets per reinforcer) delivered by two concurrent alternatives, and carried out a multiple linear regression of the log ratios of responses, reinforcer rates, and reinforcer magnitudes on his complete data set across pigeons and experimental conditions. His analysis produced a mean sensitivity to rate (a_R) of 0.60 and a mean sensitivity to magnitude (a_M) of 0.34. Todorov (1973), in a three-key procedure in which

reinforcer magnitude was defined as duration of access to the food magazine, reported a mean a_R of 0.90, and a mean a_M of 0.27. Keller and Gollub's (1977) Experiment 1 produced, according to a reanalysis reported by Davison and McCarthy (1988), group values of $a_R = 0.62$ and $a_M = 0.50$. In contrast, Keller and Gollub's Experiment 2 produced, again according to Davison and McCarthy's reanalysis, identical values of 1.06 for both sensitivities. Todorov *et al.* (1984), in a procedure in which experimental conditions changed every 8-hr session, reported values of a_R in the range 0.81 to 1.13, and of a_M in the range 0.23 to 0.62. Landon *et al.* (2003) reported a mean a_R across pigeons of 0.97 and a mean a_M of 0.76, using a procedure in which reinforcer magnitude was defined as a number of short magazine presentations in rapid succession. Davison and Baum (2003), in a procedure in which reinforcer-magnitude ratios changed several times within a session, found that a_M increased to about 0.25 after nine reinforcers within a component arranging a constant magnitude ratio. In a similar procedure in which different components arranged different reinforcer-rate ratios, Davison and Baum (2000) found that a_R increased to about 0.60 after six to eight reinforcers in a component. In general, the available data support the conclusion that Equations 2 and 3 adequately describe the effect of varying reinforcer rate and magnitude, and that sensitivity to magnitude is less than sensitivity to reinforcer rate.

Davison and Hogsden (1984) reported a data set that could not be described by Equation 3. In their Part 5, they held one reinforcer magnitude constant at 3-s access to wheat, and varied the other from 1-s to 10-s access over five conditions. The relation between log response ratios and log magnitude ratios was clearly nonlinear, with choice becoming more extreme as the total reinforcer magnitude over both alternatives increased. That is, sensitivity to magnitude depended on total reinforcer magnitude, and their data could not sensibly be fitted by Equation 3 with a constant a_M . Alsop and Elliffe (1988) and Elliffe and Alsop (1996) reported a similar challenge to Equation 2. In both experiments, a_R changed as the overall reinforcer rate provided by the concurrent schedule changed (see also Fantino, Squires, Delbrück, & Peterson, 1972; Logue & Chavarro, 1987). Williams

and Fantino (1978) found that sensitivity to reinforcer delay increased with the average delay across the concurrent alternatives.

Such results challenge a strong assumption underlying Equation 1 and the matching approach to choice: Response ratios should be affected only by the ratios of independent variables, and not by their absolute values. McLean and Blampied (2001) called this the assumption of *relativity* (see also Prelec, 1984). Equation 1 also makes a second assumption, that the effects of each IV must be independent of the values of all other IVs. McLean and Blampied called this the assumption of *independence*.

If supported by the data, the assumption of independence would greatly simplify the task of quantifying choice. For example, if we were to measure the effect of arranging unequal reinforcer rates with equal reinforcer magnitudes and, in separate conditions, the effect of arranging unequal magnitudes at equal rates, we could accurately predict choice in a condition in which both magnitudes and rates were unequal between the alternatives. However, the results of tests of the independence assumption have been equivocal.

The assumption of independence has two parts: Sensitivity to one IV should be independent of both the absolute and relative values of all other IVs. A little inelegantly, we will call these the assumptions of *absolute* and *relative independence*. Tests of absolute independence have appeared sporadically in the literature, and have produced inconclusive results. For example, Logue and Chavarro (1987) found that a_R decreased when reinforcer magnitudes on both alternatives were increased, or when reinforcer delays on both alternatives were decreased, but Davison (1983) found constant a_R across different reinforcer delays. Davison (1988) found that a_M decreased when equal reinforcer rates on both alternatives were increased, but Davison and Baum (2003) found that the level to which a_M increased towards the end of components in their within-session procedure did not depend on the overall reinforcer rate.

However, the above experiments have in general not been thorough parametric variations of two independent variables. Davison (1988), for example, arranged only two different reinforcer magnitudes and inferred changes in sensitivity to magnitude from his finding

that response ratios were less extreme when the reinforcer rate delivered at both alternatives was higher. This approach assumes a linear relationship between log response rates and log reinforcer magnitudes (Equation 3), rather than directly testing that relationship.

McLean and Blampied (2001) noted Davison's (1988) and Davison and Nevin's (1999) comment that the literature on interactions between independent variables is confused, and that future theoretical developments must await a more complete parametric investigation of possible interactions. They therefore collected a more extensive data set on the effect of varying relative reinforcer rate at different equal and unequal reinforcer magnitudes. In their Parts 1 and 2, they varied relative reinforcer rate over four levels with equal 1.5-s and 5-s reinforcer magnitudes and, using different pigeons, with equal 2-s and 6-s reinforcer magnitudes. There was no indication that a_R was affected by absolute reinforcer magnitude in either part, thus supporting the assumption of absolute independence, contrary to Davison's (1988) conclusion.

McLean and Blampied (2001) also arranged unequal 2-s and 6-s reinforcer magnitudes and varied relative reinforcer rate over four levels. The resulting estimates of a_R with unequal magnitudes were lower than those with equal 2-s reinforcers for all 4 pigeons, and lower than those with equal 6-s reinforcers for 3 of the 4 pigeons. This result suggests a violation of the assumption of relative independence, that sensitivity to one IV should be the same whether another IV is equal or unequal at the two alternatives. In their Part 3, McLean and Blampied tested this assumption more thoroughly, by varying relative reinforcer rate over five levels at three magnitude ratios, 2 s vs 6 s, 4 s vs 4 s, and 6 s vs 2 s. They found no indication that a_R was affected by the magnitude ratio.

We know of only two other planned tests of the second part of the independence assumption in simple concurrent schedules. Hunter and Davison (1982) varied both reinforcer rate and the force required for a keypeck to count as an effective response in a concurrent VI VI schedule with pigeons as subjects. They found $a_R = 0.88$ and sensitivity to response force = -0.71 . (Sensitivity to response force is negative because small force requirements are preferred over large force requirements.) Sensi-

tivity to reinforcer rate did not appear to depend on the response force ratio, and sensitivity to response force did not appear to depend on the reinforcer magnitude ratio.

Leon and Gallistel (1998) reinforced rats' lever presses with pulses of electrical brain stimulation to the lateral hypothalamus. They varied both reinforcer rate and reinforcer magnitude, defined as the frequency of pulses during a constant-duration reinforcer, and found that the two independent variables combined multiplicatively to determine the ratio of lever presses. That is, their effects were independent. However, Leon and Gallistel's design held the reinforcer rate and/or magnitude constant on one lever while varying rate and/or magnitude on the other lever. Therefore, both relative and absolute reinforcer rates and magnitudes were manipulated simultaneously. Given the doubtful status of absolute independence, this makes Leon and Gallistel's results difficult to interpret as clear evidence for relative independence, and McLean and Blampied's (2001) suggestion that more data are needed remains valid.

More data are available if we extend the generalized-matching account to include behavior on concurrent-chain schedules as well as simple concurrent schedules. Berg and Grace (2004) summarized the literature on both types of schedule, and concluded that the success of the two parts of the independence assumption differed. They found that sensitivities to reinforcer rate, magnitude, and delay were in general not independent of the absolute level of either that variable or of another of the three variables, thus failing to support absolute independence. Hunter and Davison (1982), however, did report absolute independence between reinforcer rates and response force requirements.

More to our purpose in this experiment, Berg and Grace (2004) reported that previous tests of relative independence were consistently successful. As well as the experiments described above, Grace (1995), Grace, Bedell, and Nevin (2002), and Rodriguez and Logue (1986) all found that sensitivity to reinforcer magnitude was independent of relative reinforcer delay, and Berg and Grace themselves found that sensitivity to reinforcer delay was independent of relative reinforcer rate.

The present experiment was designed to test the converse relative-independence assump-

tion from that examined by McLean and Blampied (2001): Is sensitivity to reinforcer magnitude independent of the reinforcer-rate ratio? We know of no experiment specifically designed to address this question¹. In a factorial design, we arranged five different reinforcer-magnitude ratios at each of three reinforcer-rate ratios, allowing a thorough test of the adequacy of Equation 1. Since there is a degree of inconsistency between the results of McLean and Blampied's Parts 2 and 3, we also aimed to provide further data on the independence of sensitivity to reinforcer rate from the reinforcer-magnitude ratio. We arranged three to five different reinforcer-rate ratios at each of five different reinforcer-magnitude ratios.

METHOD

Subjects

The same 5 homing pigeons used by Landon, Davison, and Elliffe (2002, 2003) served as subjects. They were numbered 131, 132, 134, 135, and 136, and were maintained at $85\% \pm 15$ g of their ad lib weights by feeding of mixed grain at about 10:00 each day. Water and grit were freely available in the subjects' cages at all times.

Apparatus

Each pigeon lived in a cage 380 mm high by 380 mm wide by 380 mm deep. The back, left and right walls of each cage were constructed of sheet galvanized iron, and the top, floor, and front wall (door) were grids of galvanized iron bars. Each cage contained two wooden perches, about 35 mm in cross section, one mounted 95 mm from and parallel to the door, and the other 95 mm from and parallel to the right wall.

The right wall of each cage contained the response interface. Three translucent keys, 20 mm in diameter, were centered 100 mm

¹ Berg and Grace's (2004) Phase 2, which was designed for another purpose, did arrange two magnitude ratios at each of three rate ratios, and reported no interaction between control by rate and magnitude. However, they ran only 4 pigeons, severely limiting power to detect any interaction, and reported estimates of sensitivity to magnitude, each based on only two data points, that were both unusually high and unusually variable. The existence of this data set does not remove the need for a more thorough manipulation of both variables.

apart and 200 mm above the perches. Only the left and right keys were used in this experiment. The left key could be lit yellow and operated as a switching key. The right (main) key could be lit red or green according to the VI schedule in operation. Pecks to lit keys of a force exceeding about 0.1 N counted as responses. A hopper containing wheat was located behind a 50-mm by 50-mm aperture centered 145 mm below the center key. During reinforcement, the key lights were extinguished and the hopper raised and illuminated. Each reinforcer consisted of a number (see Table 1) of 1.2-s hopper presentations separated by 0.5-s blackouts. An IBM® PC-compatible computer running MED-PC IV® software, located in a separate room, controlled all experimental events and recorded the time, to 10-ms resolution, at which each event occurred. No personnel entered the room containing the experimental cages while a session was running. The room lighting switched on at 00:00 and off at 16:30 each day.

Procedure

Experimental sessions were conducted daily, starting at 01:00 and were run in the order of

the subject numbers. Sessions ended in blackout after 80 reinforcers had been delivered or 42 min had elapsed, whichever occurred first. There was no stability criterion, but each experimental condition was in effect for at least 50 sessions so that we could collect enough data for detailed analyses in the manner of Davison and Baum (2000). We used the data from Sessions 16 to 50 of each condition in all analyses.

Table 1 shows the sequence of conditions. We used a switching-key concurrent VI VI schedule (Findley, 1958) throughout. Sessions began with the left (switching) key lit yellow and the right (main) key lit either red or green with equal probability. A single peck to the switching key strictly alternated both the VI schedule and the stimulus on the main key. Reinforcers were arranged by a single constant-probability VI 30-s schedule by interrogating a probability gate set to $p = .0333$ every 1 s, and then allocated to either the red or green alternative probabilistically so as to produce the reinforcer-rate ratios and VI schedules shown in Table 1. Thus, we used dependent scheduling (Stubbs & Pliskoff, 1969). A 2-s changeover delay (Herrnstein,

Table 1

Sequence of experimental conditions, showing the VI schedules (s) on the red and green alternatives, the red:green reinforcer-rate ratio, and the magnitudes of red and green reinforcers. Magnitudes are given as the number of 1.2-s magazine presentations making up a single reinforcer delivery on each alternative.

Condition	VI schedules (s)		Reinforcer rate ratio	Reinforcer magnitudes	
	Red	Green		Red	Green
1	60	60	1:1	2	6
2	60	60	1:1	6	2
3	60	60	1:1	1	7
4	60	60	1:1	4	4
5	60	60	1:1	7	1
6	33	300	9:1	7	1
7	33	300	9:1	1	7
8	33	300	9:1	6	2
9	33	300	9:1	2	6
10	33	300	9:1	4	4
11	60	60	1:1	2	6
12	300	33	1:9	7	1
13	40	120	3:1	2	6
14	120	40	1:3	2	6
15	120	40	1:3	6	2
16	40	120	3:1	6	2
17	300	33	1:9	1	7
18	300	33	1:9	6	2
19	300	33	1:9	2	6
20	300	33	1:9	4	4
21	33	300	9:1	2	6

1961) prevented main-key responses from producing a reinforcer until 2 s had elapsed since the last switching-key response.

We defined reinforcer magnitude in the same way as did Davison and Baum (2003) and Landon *et al.* (2003). Each reinforcer delivery consisted of a specified number of 1.2-s hopper presentations separated by 0.5-s black-outs. The sum of reinforcer magnitudes on the red and green alternatives was always eight hopper presentations, or 9.6-s total access to wheat. This technique was designed to counteract Epstein's (1981) finding that the amount of food consumed from a magazine is not a constant proportion of the duration of access, and to ensure that the magazine's wheat receptacle would not be emptied even during a reinforcer of long duration (Epstein, 1985). In different conditions, we arranged red/green reinforcer-magnitude ratios of 7:1, 6:2, 4:4, 2:6, and 1:7. The overall reinforcer rate made available by the concurrent VI VI schedule was always two reinforcers/min. Across conditions, we arranged red/green reinforcer ratios of 9:1, 3:1, 1:1, 1:3, and 1:9. We chose the conditions so as to provide an extensive set of combinations of both equal and unequal reinforcer rates and magnitudes on the red and green alternatives (Table 1). Conditions 1 through 5 were also reported by Landon *et al.* Condition 11 replicated Condition 1, and Condition 21 replicated Condition 9.

RESULTS

The Appendix shows the numbers of responses emitted, time spent responding, and reinforcers obtained on the red and green alternatives, and the number of switching-key responses, for each pigeon in each condition. To promote comparability of data between conditions with different numbers of sessions, these data are summed over Sessions 16 through 50 in each condition, even when more than 50 sessions were run in a condition. As a preliminary check on the consistency of data throughout these 35 sessions, we calculated \log_{10} red/green response ratios for Sessions 16 through 32 (i.e., for a typical condition duration in previous concurrent-schedule research) and, separately, for Sessions 33 through 50 for each pigeon in each condition. This analysis is shown in Figure 1. It

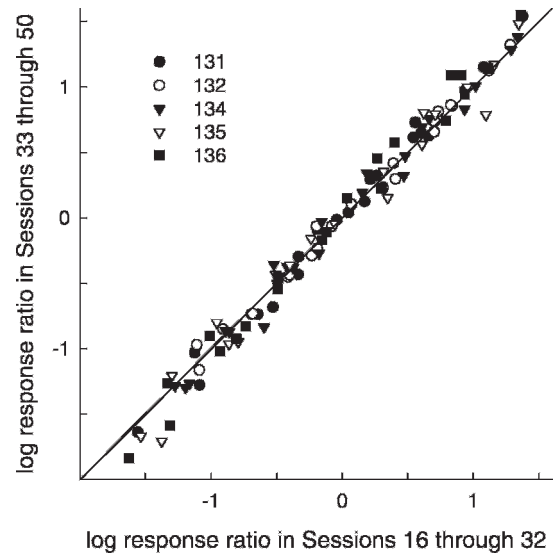


Fig. 1. \log_{10} red/green response ratio in Sessions 33 through 50 as a function of \log_{10} red/green response ratio in Sessions 16 through 32 for each pigeon in each condition. The line shows the major diagonal, or equality between the log ratios in each block of sessions.

is clear that log response ratios during these different parts of each condition were very highly correlated (Pearson's $r = 0.99$). Choice may have become slightly more extreme as training in a condition continued (see also Todorov, Oliveira Castro, Hanna, Bittencourt de Sá, & Barreto, 1983), but any effect is very small, and we are clearly justified in treating behavior throughout the 35 sessions used for analysis in each condition as homogenous.

Figure 2 shows a generalized-matching analysis of the effects of reinforcer magnitude on preference at three different reinforcer-rate ratios. The figure plots \log_{10} red/green response ratios as a function of \log_{10} red/green reinforcer-magnitude ratios, calculated as log ratios of the numbers of 1.2-s hopper presentations on the red and green alternatives. The data are plotted separately for conditions arranging red/green reinforcer-rate ratios of 9:1 (Conditions 6 through 10, and 21), 1:1 (Conditions 1 through 5, and 11), and 1:9 (Conditions 12, and 17 through 20). Condition 9 and its replication, Condition 21, produced similar data. Lines fitted to the data at each reinforcer-rate ratio separately denote the best least-squares fit of Equation 3. Table 2 gives the parameters of (a_M and $\log c$) and percentage of variance accounted for (r^2) by

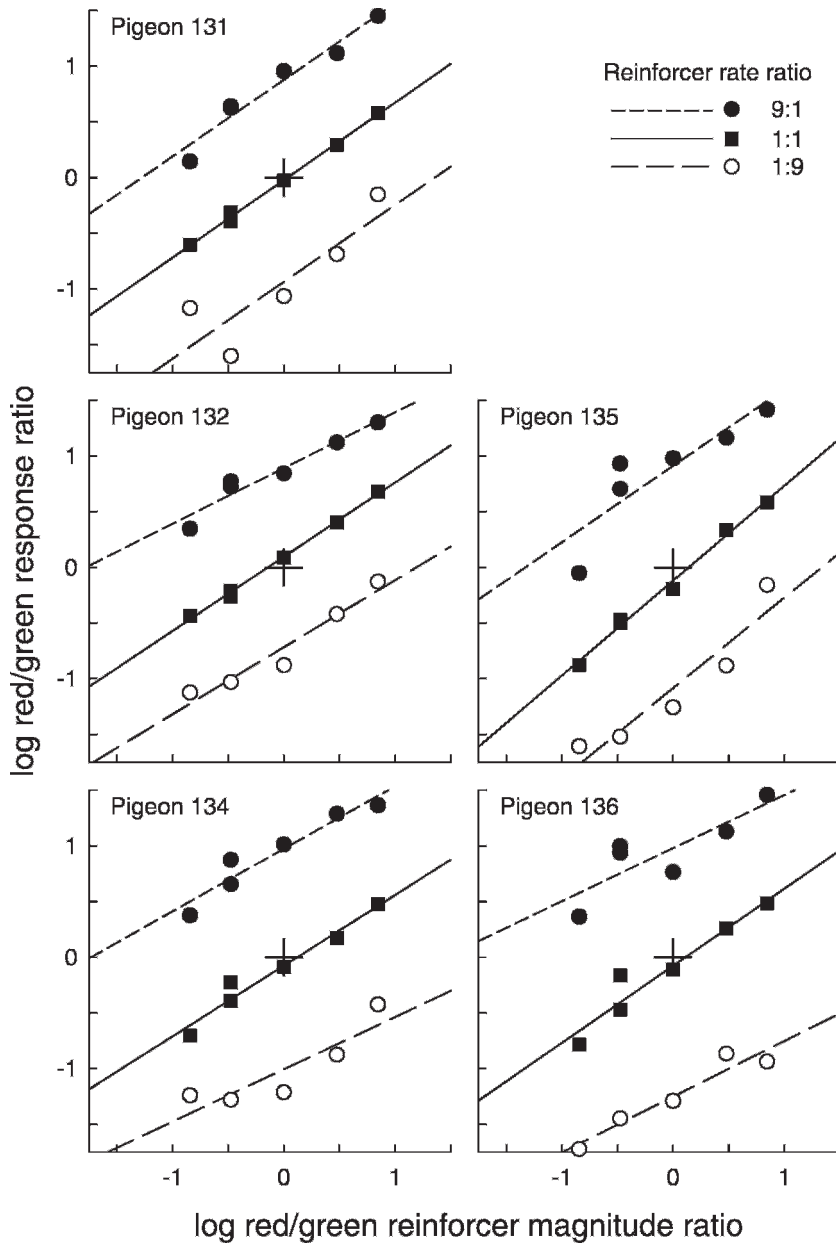


Fig. 2. \log_{10} red/green response ratio as a function of \log_{10} reinforcer-magnitude ratio for each pigeon. The straight lines show the best least-squares fits of Equation 3. Different symbols and line types denote data collected at different reinforcer-rate ratios, as shown in the legend.

these fits, and the means of these values across pigeons.

Figure 2 and Table 2 show that choice was strongly biased towards the alternative delivering more frequent reinforcers, and showed little bias when the reinforcer-rate ratio was 1:1. Across pigeons, estimates of $\log c$ in

Equation 3 were perfectly ordered with respect to the reinforcer-rate ratio (Friedman analysis of variance by ranks; $\chi^2_r = 10.0$, 2 *df*, $p < .05$). Equation 2 described choice very well when the reinforcer-rate ratio was 1:1 (mean $r^2 = 97\%$, range 93% to 100%), but less well when the red and green alternatives delivered

Table 2

Least-squares fits of Equation 3 (generalized matching to reinforcer magnitudes) and Equation 2 (generalized matching to reinforcer rates). The slope a is sensitivity to that independent variable, the intercept $\log c$ is bias, and r^2 is the percentage of data variance accounted for by the fitted line.

Pigeon		Equation 3 (magnitude)			Equation 2 (rate)				
		Reinforcer-rate ratio			Reinforcer-magnitude ratio				
		1:9	1:1	9:1	1:7	2:6	4:4	6:2	7:1
131	a	0.69	0.69	0.69	0.76	1.09	1.05	0.91	0.78
	$\log_2 c$	-0.94	-0.02	0.88	-0.58	-0.35	-0.05	0.30	0.62
	r^2	75%	100%	95%	100%	99%	100%	99%	100%
132	a	0.60	0.67	0.50	0.79	0.93	0.95	0.84	0.77
	$\log_2 c$	-0.72	0.10	0.89	-0.42	-0.19	0.01	0.36	0.63
	r^2	94%	100%	93%	100%	99%	100%	99%	100%
134	a	0.47	0.63	0.56	0.87	1.06	1.16	1.11	0.90
	$\log_2 c$	-1.01	-0.08	0.97	-0.53	-0.30	-0.07	0.17	0.47
	r^2	78%	96%	92%	98%	98%	100%	100%	100%
135	a	0.81	0.85	0.69	0.78	1.25	1.15	1.10	0.81
	$\log_2 c$	-1.08	-0.12	0.91	-0.82	-0.40	-0.18	0.22	0.60
	r^2	89%	99%	77%	100%	99%	100%	100%	100%
136	a	0.50	0.69	0.48	1.15	1.25	1.11	1.13	1.18
	$\log_2 c$	-1.25	-0.08	0.98	-0.74	-0.31	-0.22	0.21	0.32
	r^2	82%	93%	70%	100%	98%	100%	98%	99%
Mean	a	0.61	0.71	0.58	0.87	1.12	1.08	1.02	0.89
	$\log_2 c$	-1.00	-0.04	0.94	-0.62	-0.31	-0.10	0.25	0.53
	r^2	86%	97%	85%	99%	99%	100%	99%	100%

unequal reinforcer rates (mean $r^2 = 86\%$, range 70% to 95%). A Friedman ANOVA confirmed that the quality of the fit of Equation 3 depended on the reinforcer-rate ratio, $\chi_r^2 = 7.6$, 2 df , $p < .05$.

The slopes of the fitted lines in Figure 2 show sensitivity to reinforcer magnitude (a_M in Equation 3). Estimates of a_M ranged from 0.47 to 0.86 with a mean of 0.63. Figure 3 shows

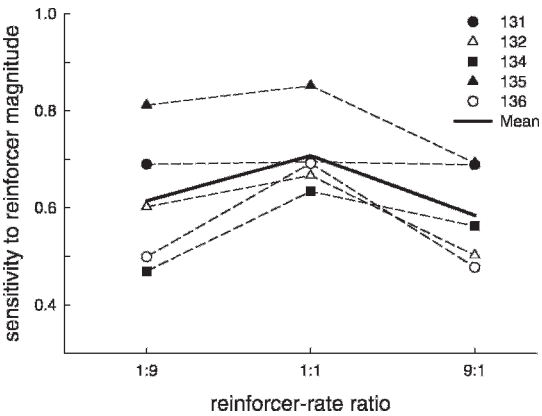


Fig. 3. Sensitivity to reinforcer-magnitude ratio (a_M in Equation 3) as a function of the arranged reinforcer-rate ratio. Different symbols show values for each pigeon, and the heavy solid line shows the means of the individual sensitivities.

these estimates as a function of the arranged reinforcer-rate ratio. Both Figures 2 and 3 show that sensitivity to reinforcer magnitude appears to be greater when the red and green alternatives delivered equal, rather than unequal, reinforcer rates. This conclusion was supported by a Friedman ANOVA, $\chi_r^2 = 8.4$, 2 df , $p < .05$. Post hoc pairwise contrasts (Marascuilo & McSweeney, 1977) showed that the apparent difference between sensitivities to magnitude when the reinforcer-rate ratio was 9:1 versus 1:1 was significant at $p < .05$.

Figure 4 shows a generalized-matching analysis of the effects of reinforcer-rate ratio at five different reinforcer-magnitude ratios. The figure plots \log_{10} red/green response ratios as a function of \log_{10} red/green reinforcer-rate ratios, separately for sets of conditions arranging red/green reinforcer-magnitude ratios of 7:1 (Conditions 5, 6, and 12), 6:2 (Conditions 2, 8, 15, 16, and 18), 4:4 (Conditions 4, 10, and 20), 2:6 (Conditions 1, 9, 11, 13, 14, 19, and 21), and 1:7 (Conditions 3, 7 and 17). Conditions 11 and 21 provided good replications of Conditions 1 and 9 respectively.

The lines show the best least-squares fits of Equation 2 to the data at each magnitude ratio separately, and the parameters of and variance accounted for by each fit are shown in Table 2.

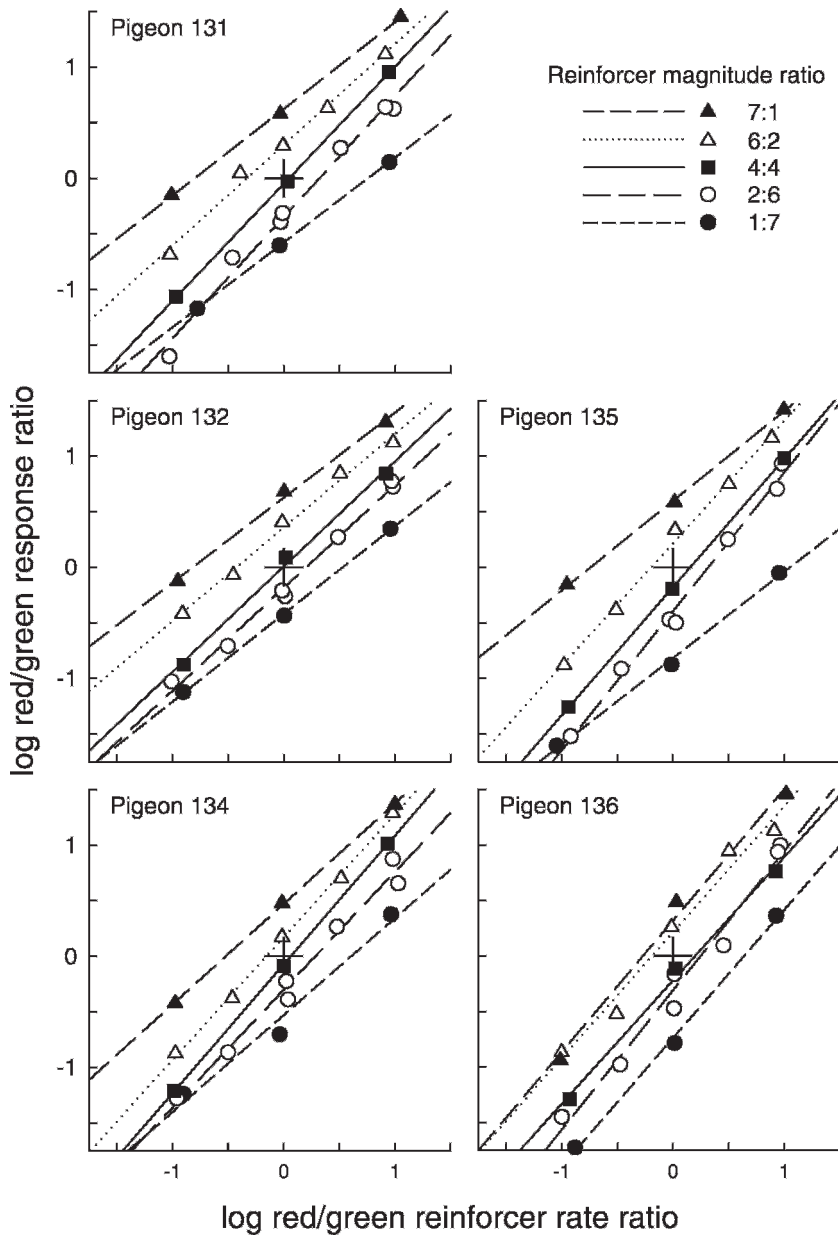


Fig. 4. \log_{10} red/green response ratio as a function of \log_{10} reinforcer-rate ratio for each pigeon. The straight lines show the best least-squares fits of Equation 2. Different symbols and line types denote data collected at different reinforcer-magnitude ratios, as shown in the legend.

Equation 2 described the data extremely well at all magnitude ratios (mean $r^2 = 99\%$, minimum = 98%). Estimates of bias ($\log c$ in Equation 2) were perfectly ordered for all pigeons with respect to the reinforcer-magnitude ratio, Friedman $\chi_r^2 = 20.0$, 4 df , $p < .05$.

The slopes of the fitted lines in Figure 4 show sensitivity to reinforcer rate (a_R in Equation 2). Estimates of a_R ranged from 0.76 to 1.25 with a mean of 1.00. Sensitivity to reinforcer rate was consistently greater than sensitivity to reinforcer magnitude (Mann-

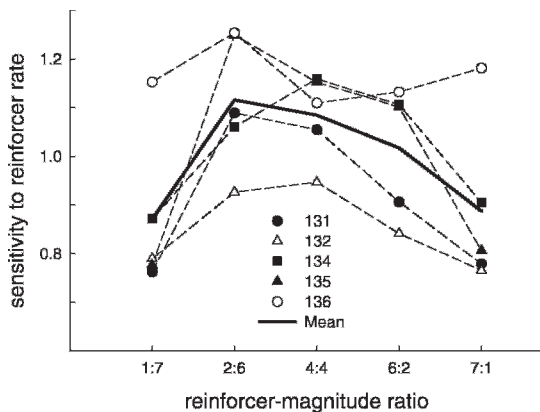


Fig. 5. Sensitivity to reinforcer-rate ratio (a_R in Equation 2) as a function of the arranged reinforcer-magnitude ratio. Different symbols show values for each pigeon, and the heavy solid line shows the means of the individual sensitivities.

Whitney U , $z = 4.88$, $p < .05$). Figure 5 plots a_R as a function of the arranged reinforcer-magnitude ratio, and shows that sensitivity to rate was not constant across magnitude ratios, Friedman $\chi_r^2 = 8.4$, 4 df , $p < .05$. Post hoc tests revealed no significant pairwise contrasts, but it appears from Figures 4 and 5 that sensitivity to reinforcer rate generally decreased as the reinforcer magnitudes on the red and green alternatives became more unequal.

We carried out parallel generalized-matching analyses using log time-allocation ratios rather than log response ratios as the measure of choice. All conclusions were identical, although time-allocation sensitivities to both rate and magnitude were consistently higher than those for response-allocation measures (Elliffe & Alsop, 1996; Landon et al., 2003; Taylor & Davison, 1983). In the interests of saving space, we have not presented the analyses of time allocation here. We also repeated the hypothesis tests using repeated-measures parametric ANOVAs. Again, all conclusions were identical to those of the nonparametric Friedman tests reported here.

Figure 2 and Table 2 show that Equation 3 described the effect of varying reinforcer-magnitude ratio less well when the reinforcer rates were unequal on the red and green alternatives than when they were equal. It appears from Figure 2 that there may be systematic deviations from the fitted lines with unequal reinforcer rates, with Equation 3

underpredicting the strength of preference when reinforcer magnitudes were unequal, and overpredicting the strength of preference with unequal magnitudes. To assess this possibility, we analyzed the residuals of the least-squares fits of Equation 3 to the data obtained at 9:1 and 1:9 reinforcer-rate ratios in Figure 2. When the reinforcer-rate ratio was 1:9, the residuals were not significantly related to the reinforcer-magnitude ratio, Friedman $\chi_r^2 = 6.56$, 4 df , $p > .05$. However, when the reinforcer-rate ratio was 9:1, the data did deviate systematically from the fitted line, Friedman $\chi_r^2 = 14.72$, 4 df , $p < .05$, suggesting that Equations 1 and 3 may fail systematically to describe choice data when reinforcer magnitudes are unequal.

We conducted two further analyses, an ANOVA on log response ratios in each condition, and a model comparison using the Schwarz-Bayes information criterion. Values of the information criterion are meaningless in absolute terms—they can only be compared for different models of the same data set—and so there is little point in listing them, and the reason for the ANOVA will more naturally become apparent later, so we will defer consideration of these analyses until the Discussion.

DISCUSSION

One potential confound needs to be considered. Although our procedure has some precedent in this literature (Davison & Baum, 2003; Landon et al., 2003) perhaps our manipulation of reinforcer magnitude via the number of brief hopper presentations is better considered as a manipulation of reinforcer rate. If that were the case, an interaction between rate and magnitude might not be surprising. We think this possibility is very unlikely, however. The keys darkened during the 0.5-s intervals between 1.2-s hopper presentations and, while pecks to the dark keys during these periods could have been recorded by the experimental control software, they never occurred. That is, the reinforcement period cannot sensibly be seen as a period during which responses were reinforced at a very high rate. Informal observations during training suggested that the pigeons kept their heads in or very close to the magazine aperture throughout reinforcer delivery. Taken together,

er, these points suggest strongly that the rapid series of brief hopper presentations did indeed function as a single reinforcer and that variations in the length of that series are, as we intended, better conceptualized as magnitude manipulations than rate manipulations.

Changes in Best-Fitting Estimates of Sensitivity

On the face of it, our data pose the first substantial challenge to the second, or relative, part of the independence assumption: Sensitivity to reinforcer magnitude (a_M in Equation 3) did depend systematically on the reinforcer-magnitude ratio (Figures 2 & 3), and sensitivity to reinforcer rate (a_R in Equation 2) similarly depended on the reinforcer-magnitude ratio (Figures 4 & 5). In both cases, sensitivity to one independent variable was greatest when the other independent variable was equal at the two alternatives.

Our finding that a_R was not independent of relative reinforcer magnitude is contrary to that of McLean and Blampied (2001). This may have been due to the more powerful design of the present study: We used more pigeons, more and a wider range (1:7 through 7:1 rather than 2:6 through 6:2) of magnitude ratios, and a wider range of rate ratios (1:9 through 9:1 rather than 1:4 through 3:1). Arguably, we therefore explored more of the parameter space than did McLean and Blampied.

As did Leon and Gallistel (1998), McLean and Blampied (2001) manipulated the reinforcer-rate ratio by holding rate on one alternative constant while varying the other. As discussed earlier, this design changes both relative and absolute reinforcer rates simultaneously. The lack of independence of sensitivity to rate from absolute rate is already established (Alsop & Elliffe, 1988; Elliffe & Alsop, 1996; see also Berg & Grace, 2004). It may be that the design of the present study, which held absolute reinforcer rate constant, more effectively allowed a relation between a_R and relative magnitude to become apparent, because the potential confounding variable of changes in absolute reinforcer rate was eliminated.

Although the independent variables concerned are different, our result is also inconsistent with those of Grace (1995) and Grace et al. (2002), who reported that a_M was indepen-

dent of relative reinforcer delay on concurrent-chain schedules, and Berg and Grace (2004), who reported similar independence of sensitivity to reinforcer delay from relative reinforcer rate. There are several possible explanations for this difference. First, it may simply be that the relative-independence assumption applies to some variables that control choice, but not to others. Control by reinforcer delay may indeed be independent of other variables, whereas control by reinforcer rates and magnitudes is not. Second, it may be that the assumption is valid for concurrent-chain schedules, but not for simple concurrent schedules. Third, there is a potentially important difference in design between Grace's series of experiments and the present study.

In the experiments listed above, Grace and colleagues (Grace, 1995; Grace et al., 2002; Berg & Grace, 2004) arranged three-component multiple concurrent-chain schedules, in which one independent variable was varied across conditions, while the other was varied within sessions and signaled by the color of the keys during the initial links. That approach has the advantage of allowing a within-session measure of control by one independent variable, and so of comparing data collected under different levels of an independent variable at the same time. It also substantially shortens the time required to conduct an experiment. However, it has the disadvantage of requiring the assumption that stimulus control by the initial-link stimuli has fully developed, and remains constant, throughout the experiment. Krägeloh, Elliffe, and Davison (2006) showed that this was not true in an admittedly more complex procedure that arranged seven reinforcer ratios during each session, each signaled by a discriminative stimulus. Stimulus control was not complete at the beginning of each signaled component, but rather continued to develop with continued exposure to the reinforcer ratio. The extent of stimulus control at the beginning of a component depended on the simplicity or otherwise of the relation between the stimulus dimension and the set of reinforcer ratios that it signaled. It is possible that requiring control by one of the independent variables under investigation to depend on acquisition of control by the stimulus signaling that variable confuses the situation enough to obscure any interaction between control by that indepen-

dent variable and another. Stimulus control requires both a stimulus and a reinforcer differential to develop—thus, if it does not develop, or is incomplete, it is impossible to know whether reinforcer control, stimulus control, or both are being measured. The situation is rendered even more complex when the reinforcer differential is both on the magnitude and delay dimensions.

A Model-Comparison Approach

To this point, we have emphasized the differences in the best-fitting estimates of sensitivity to one independent variable across changes in the ratio of another independent variable. These changes are apparent in Figures 2 and 4, and were confirmed by hypothesis tests. Clearly, this is one tenable way of testing the relative-independence assumption that underlies concatenated generalized matching. There are other possible approaches to that issue, however.

A natural question to ask is whether, despite the significant differences in sensitivity values, a model preserving the relative-independence assumption describes our data adequately. The simplicity of Equation 1 is appealing, and it could be argued that a move to a more complex description, in which sensitivity to an independent variable does not remain constant across changes in another independent variable, needs to be justified by a substantial improvement in the precision of the empirical description of the data. This argument is at the heart of the model-comparison approach (e.g., Burnham & Anderson, 2002). The approach incorporates several possible techniques (e.g., Elliffe, 2006), but its essence is a tradeoff between the goodness-of-fit of a model and the number of free parameters the model contains. Good models, according to this criterion, describe the data acceptably well with few parameters.

One way of assessing this tradeoff quantitatively is the Schwarz-Bayes information criterion (Schwarz, 1978). We chose this criterion because it penalizes extra free parameters a little more strictly than does the Akaike criterion (Akaike, 1969, 1974). Assuming that errors of prediction are normally distributed, the criterion (*SIC*) is:

$$SIC = N \ln \frac{RSS}{N} + k \ln N, \quad (4)$$

where N is the number of data, k is the number of parameters to be estimated, and RSS is the total error of prediction, measured as the residual sum of squares. A smaller value of *SIC* indicates the model to be preferred, because *SIC* decreases with decreasing RSS , and increases with increasing k .

We calculated the Schwarz-Bayes criterion for the data shown in Figure 2 and for two models. One, the independent-slopes model, is exactly the analysis shown in Figure 2 and Table 2. It allows different slopes, or values of a_M , at each reinforcer-rate ratio, and represents a violation of the relative-independence assumption. The other, the common-slope model, constrained a_M to be the same at all rate ratios. That is, it applied Equation 1 directly, and represents maintenance of the relative-independence assumption. For all 5 pigeons, *SIC* was slightly smaller for the common-slope model, indicating that the improvement in fit conferred by abandoning relative independence was not enough to justify the extra two free parameters (i.e., three values of a_M for the three reinforcer-rate ratios). A similar analysis of the data in Figure 4 showed that, according to *SIC*, forcing a common a_R at all reinforcer-magnitude ratios described the data more efficiently than allowing different values of a_R at each magnitude ratio. This conclusion parallels that of, for example, Berg and Grace (2004), who quantified the tradeoff between improved empirical description and extra free parameters in a different way. They tested the significance of the incremental variance accounted for by allowing independent sensitivities to reinforcer delay at each of three reinforcer-rate ratios, compared to forcing a common sensitivity to delay, and similarly concluded that the improvement in goodness-of-fit did not justify the extra two free parameters.

A model-comparison approach therefore suggests a different conclusion than did our initial report of systematic differences in the best-fitting estimates of sensitivity: The improvement in empirical description was not enough to justify a more complex model, and the simplifying assumption of relative independence can be maintained. However, we are unconvinced by this analysis for two reasons. First, our Schwarz-Bayes calculations were necessarily carried out on each pigeon's data

independently. This means that the replication of an effect across subjects, which clearly influenced the results of our initial hypothesis tests, carried no weight in the model-comparison analysis. (There is, as yet, no well-known and simple method of combining information criteria across subjects.) Each subject's data were therefore required to pass the Schwarz-Bayes test independently, and this seems an unreasonably stringent test. The power of replication is lost entirely, and the chances of detecting a small, but reliable, across-subjects effect are greatly reduced. This criticism applies equally to Berg and Grace's (2004) use of incremental variance accounted for, which is similarly applied to each of their subjects independently.

Second, and more easily addressed, an analysis allowing three separate sensitivities to magnitude, one for each rate ratio, may be an unnecessarily extreme violation of the relative-independence assumption. We arranged one set of conditions with equal reinforcer rates on each alternative (a 1:1 ratio), and two others with unequal but symmetrical reinforcer rates (1:9 and 9:1 ratios). A more sensible violation of the assumption might be that a_M changes systematically as the rate ratio departs from 1:1, in either direction. There seems no reason why, for example, 1:9 and 9:1 rate ratios should produce different values of a_M , since they are equivalent departures from equal reinforcer rates.

We therefore repeated the model-comparison analysis, but this time compared the common-slope model (i.e., Equation 1) with a two-slope model in which a_M was constrained to be equal for both the 1:9 and 9:1 rate ratios, but was free to vary from this value at the 1:1 rate ratio. Values of *SIC* for the two models were very similar for all pigeons, with the two-slope model being slightly preferred (smaller *SIC*) for Pigeons 132 and 136, and the common-slope model performing better for the other 3 pigeons' data.

Adding a principled constraint—that control by reinforcer magnitude should not be differentially affected by departures from equal reinforcer rates of the same size but in opposite directions—greatly weakens the case for the relative-independence assumption provided by the model-comparison approach. On this basis, there are no clear grounds for

preferring either the simple or the more complex model.

Finally, we explored, independently of the generalized-matching analysis, whether the effects on choice of reinforcer rates and magnitude were independent or interacted. We conducted a repeated-measures ANOVA on the mean log response ratios in each condition, with reinforcer-magnitude ratio and (necessarily, for obvious reasons) arranged reinforcer-rate ratio as within-subjects factors. Unsurprisingly, the main effects of both magnitude, $F(4, 16) = 121$, $p < .001$, and rate, $F(2, 8) = 323$, $p < .001$, ratio were significant. Despite the relative lack of power of this nondirectional test, the interaction between magnitude and rate was also significant, $F(8, 32) = 6.78$, $p < .001$.

Conclusions

We assessed independence of control of concurrent choice by reinforcer rate and magnitude ratios in three different ways. The best-fitting values of sensitivity to each independent variable changed systematically with the ratio of the other variable, suggesting that the relative-independence assumption does not hold. However, adding the constraint that each sensitivity value must remain constant had little effect on goodness-of-fit, and therefore on accuracy of prediction. As assessed by the more reasonable of the two applications of the Schwarz-Bayes criterion that we tried, there is no clear reason either to retain or to reject the relative-independence assumption. To help resolve this contradiction between the conclusions of the previous analyses, and despite sharing readers' likely misgivings about the value of omnibus ANOVAs, we did find a significant interaction between the effects of the two independent variables on choice when no model was fitted to the data.

The results of two different kinds of hypothesis test were consistent, while the results of the model-comparison approach were equivocal. On balance, therefore, we conclude that reinforcer rate and magnitude, at least when magnitude is manipulated in the manner we used, do not control choice independently. Two questions remain: First, why is this conclusion inconsistent with Berg and Grace's (2004) summary of the previous literature? We have suggested some procedural reasons that this may be so, and also point to

the relative lack of power to detect violation of the independence assumption in some of the earlier research. We suggest that the present data set is unusually comprehensive, and therefore not lightly dismissed, despite its disagreement with previous results.

Second, does it matter? The answer to this question may be as much a matter of taste as anything, and reflect different perceptions of the purpose of our discipline. If the goal is one of behavioral engineering, in the sense of prediction and control of behavior, it may not matter. In terms of efficiency of prediction, there is little to choose between the common-sensitivity and independent-sensitivity models that we analyzed, so that retaining this simplifying assumption of relative independence is justified. Equation 1 will continue to predict choice well enough for all practical purposes, without introducing the complicating factor of interaction between sources of control. On the other hand, if our goal is to understand the mechanisms underlying choice, our view is that we must accept the complication. We see concatenated generalized matching less as a theory of behavior than as a very useful shorthand description. Models that implicate a mechanism, such as melioration, contingency discriminability, or fix-and-sample, are often evaluated in part on their ability to predict a generalized-matching description of choice. Our results suggest that relative independence should not be taken as an outcome that these, more principled, theories should be expected to predict. Further, if we are correct, more extreme reinforcer-rate ratios and reinforcer-magnitude ratios will produce greater deviations from a single-sensitivity model, and thus may well begin to matter in the prediction and control of behavior.

Finally, findings of invariance, or independence, have great potential to advance science, and the search for invariance is perhaps our most important task (see Nevin, 1984). With that importance comes the responsibility to ensure that claims of invariance are robust. Such claims are major theoretical statements, at least as much so as claims of effect, or significant difference. We should therefore impose on them the same burden of proof that we do on reports of differences. It is important that invariances should not simply be accepted by default when a hypothesis test

fails to reach significance, especially in an experiment that lacks power. Rather, they must prove their existence as does any other strong theoretical position. It is our view that the claim of relative independence has not passed that test.

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APPENDIX

Numbers of responses emitted, time spent responding (s), and numbers of reinforcers obtained on the red and green alternatives, and number of switching-key responses, for each pigeon summed over Sessions 16 to 50 in each experimental condition.

Condition	Responses		Time (s)		Reinforcers		Switches
	Red	Green	Red	Green	Red	Green	
Pigeon 131							
1	9848	24390	5950	33295	848	909	5890
2	27072	13787	32807	9144	983	999	6944
3	9724	39078	5482	47564	878	959	5141
4	13413	14235	14761	15517	972	903	6312
5	33717	8882	45685	4332	905	974	5887
6	28872	1021	57934	412	1524	136	1137
7	19343	13836	24248	19423	1912	215	6139
8	27448	2101	55011	1039	1645	201	1605
9	41118	9766	48483	4501	2098	216	5397
10	37200	4123	52821	1827	1906	216	2745
11	12406	25584	9696	32410	906	932	5763
12	11047	15661	14198	19200	176	1799	6722
13	20662	11030	23882	10509	1326	408	5179
14	6180	31967	3857	48292	474	1370	3059
15	22096	19936	24711	16318	583	1438	6857
16	23690	5530	42261	3095	1201	485	3960
17	1935	28783	1279	52769	230	1382	1560
18	5360	26085	3354	42840	194	2058	4058
19	773	30924	461	53041	155	1659	919
20	1737	20162	1381	47927	173	1611	1397
21	31316	7163	46043	5382	1964	240	4052
Pigeon 132							
1	19929	36512	9507	40307	962	937	5930
2	41521	16382	42812	10144	928	960	5429
3	14339	39091	7844	47486	917	911	4792
4	29406	23996	25400	20862	1027	984	6747
5	50987	10683	54903	4843	904	898	4758
6	45784	2285	60652	846	1595	194	1482
7	36276	16351	43987	13678	2049	226	5633
8	42851	3232	58528	1625	1681	175	1586
9	38921	7366	55955	4525	2037	213	3178
10	45671	6532	57565	3202	1883	227	2747
11	20654	33559	17253	36924	970	1006	5086
12	20436	27441	23935	29239	220	1982	6166
13	28071	15018	38016	15956	1578	512	4416
14	7139	36429	4567	49832	439	1395	2723
15	19758	23304	21640	28954	517	1472	4919
16	36035	5150	54750	3333	1319	411	2135
17	2811	37487	1578	56875	192	1545	1025
18	11858	31174	10565	43555	250	2039	3964
19	3454	37060	1519	53666	177	1810	1459
20	3366	25423	2526	51009	210	1674	1567
21	38637	6511	55853	6140	2003	216	2628
Pigeon 134							
1	5874	14394	8310	35273	756	694	3097
2	10952	7339	28784	14481	747	773	2774
3	3586	18116	3589	53474	610	663	2130
4	8620	10528	15024	23383	817	820	3290
5	12892	4303	48195	6389	610	631	1849
6	12731	549	56022	568	1209	121	475
7	11681	4909	29996	18448	1475	160	1920
8	14012	722	51494	2088	1312	138	611
9	18962	2528	47961	2564	1788	187	1760
10	15400	1495	50413	1282	1472	172	1291

APPENDIX

(Continued)

Condition	Responses		Time (s)		Reinforcers		Switches
	Red	Green	Red	Green	Red	Green	
11	5402	9089	8157	30008	717	682	2685
12	4380	11606	17043	27136	152	1439	2118
13	9339	5056	27553	12656	1196	397	2167
14	1851	13579	2857	46743	340	1075	1395
15	4093	9790	10195	25955	390	1128	2359
16	5264	1052	41266	2454	619	188	698
17	740	12878	813	51998	157	1253	782
18	1994	14985	3720	45074	167	1573	1471
19	1080	20562	872	52666	163	1488	1027
20	1112	18112	1360	49353	153	1483	1035
21	16409	3635	45002	6441	1773	166	1932
Pigeon 135							
1	5836	17248	8436	27506	615	662	2324
2	17740	8217	32078	7682	801	767	2990
3	6222	46663	5830	49782	795	822	2988
4	14585	22917	18397	24111	913	925	3845
5	27417	7153	46250	4835	789	762	2499
6	35965	1381	52750	674	1479	149	741
7	12852	14468	19451	20817	1405	156	2348
8	30625	2097	47654	848	1386	179	1188
9	34396	4012	50543	2413	1809	190	1958
10	29262	3065	51191	1409	1722	172	1609
11	8912	28063	11602	34822	845	799	2881
12	16432	23619	24007	22570	160	1440	2504
13	24208	13568	32769	13373	1433	462	2954
14	5110	41946	3195	48775	450	1319	2530
15	9067	21927	10148	29256	432	1410	3389
16	23484	4199	43576	2074	1235	391	2326
17	1237	49846	660	56842	134	1486	912
18	3391	25768	3784	46219	190	1816	1590
19	1383	45702	474	56288	184	1536	915
20	1324	23978	1202	54221	169	1478	856
21	34426	6798	45140	7620	1866	217	1928
Pigeon 136							
1	8983	26591	3821	27910	929	910	6262
2	24693	13575	21168	12109	911	939	4722
3	6039	36667	2387	45180	928	898	4398
4	16076	20862	13390	22783	920	875	3892
5	32676	10744	33048	4527	974	908	5813
6	27201	950	57301	253	1541	148	960
7	26490	11459	33375	13103	1823	215	4643
8	34089	2538	50882	779	1677	206	1685
9	35568	3574	48699	1530	1871	203	2219
10	36603	6295	45119	1879	1846	220	3421
11	12201	17745	12866	19594	877	852	4585
12	3290	28534	3710	44839	161	1656	1814
13	20132	16140	22698	15415	1438	507	4733
14	3952	37501	2025	47390	453	1358	2226
15	9831	32570	8967	34177	462	1492	3660
16	33025	3744	45875	1437	1308	411	2502
17	731	38561	546	57415	162	1233	968
18	5747	42179	3158	48337	190	1907	2382
19	1248	34974	461	54047	157	1569	787
20	1766	34410	691	52674	182	1547	1130
21	44303	5111	49116	4290	1843	210	2388